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found and sudden metamorphoses. In these cases a particular activity, on which must often depend the life of the individual or of its progeny, has to be performed with a high degree of proficiency at its very phylogenetic inception or it can be of no advantage to the individual or the race. Such cases, with which you are all sufficiently familiar, have ever been the insurmountable obstacle to the evolution of instincts on the theory of fluctuating variations and natural selection. The theory of organic selection seems to me merely to conceal but not to overcome the difficulties. The mutation theory frankly avoids the difficulties even if it fails to throw any light on the origin of the mutations, and bundles this into the germ-plasma. It is, of course, no objection to the theory that it leaves something under the heavens to be accounted for. This is rather to be regarded as one of its chief virtues. As working naturalists we have reason to be most suspicious of the theories that explain everything.

*Discontinuous Variation and the Origin of Species:** Dr. D. T. MACDOUGAL, New York Botanical Garden.

That distinct and separate qualities expressed in recognizable external characters may appear suddenly, or disappear completely, in a series of generations of plants, has been a matter of common observation so long that it would be difficult to hunt out and fix upon the first instance of record.

The significance of such phenomena was obviously beyond the comprehension of the earlier botanists, and it is evident that a rational recognition of the phylogenetic value of sports and anomalies necessarily awaited the development and realization

* See also, MacDougal, D. T., 'Discontinuous Variation and the Origin of Species.' *Torreya*. 5: Jan., 1905. Pp. 1-6.

of the conceptions of unit-characters, of the minute structures which are the ultimate bearers of heredity, and of the interdependence of the two in such manner as to constitute actual entities as embodied in Darwin's pangenesis, de Vries' intra-cellular pangenesis and in Mendel's investigations upon heredity. It is equally apparent that a proper interpretation of the facts in question, and their distinction from the results of hybridization were possible only by means of the analysis of the collated results of observations upon series of securely guarded pedigree-cultures, in which the derivation of all of the individuals of several successive generations had been noted. For it is now thoroughly realized that the main questions of descent and heredity and of evolution in general are essentially physiological, and as such their solution is to be sought in experiences with living organisms and not by deductions from illusory 'prima facie' evidence, which has been so much in vogue in evolutionary polemics, nor by 'interpretations of the face of nature' with the accompanying inexact methods and superficial considerations. It was upon the safe basis of the first-named conceptions, and by means of the methods entailed, that de Vries so successfully grappled with the problems involved in the investigation of the part played by discontinuous variation in evolution.

In view of the amount of orderly and well-authenticated evidence now at hand, it may be regarded as demonstrated that characters, and groups of characters, of appreciable physiological value, originate, appear in new combinations or become latent, in hereditary series of organisms, in such manner as to constitute distinct breaks in descent.

This is the main thesis of the mutation theory—the saltatory movements of characters, regardless of the taxonomic value of the resultant forms. That the derivatives

might be considered as species by one systematist, and varieties by another, is quite incidental and of very little importance. The main contention lies in the claim that characters of a definite nature appear, and become inactive suddenly, and do not always need thousands of years for their infinitely slow external realization, or for their gradual disappearance from a strain.

Of course the principal corollary of the mutation-theory is that the saltations in question do result in the production of new species and varieties. As a matter of interest it may be stated that the systematists who have seriously examined the adult mutants of the evening-primroses cultivated in the New York Botanical Garden have unanimously held the opinion that certain ones were to be considered as species and others as varieties.

Furthermore, these conclusions are confirmed when the characters of the mutants are subjected to statistical methods of investigation. In the observations of Dr. Shull, which will be presented more fully before the Botanical Society of America, it has been found that qualities of the mutants, susceptible of measurement, depart definitely and clearly from the parent-type and fluctuate about a new mean, and do not intergrade with the parental form. The amplitude of fluctuation about the new center is greater than that of correspondent parental qualities, and the degree of correlation is much less in the mutants than in the parent. This is seen by inspection to be true in one species during the first year of its existence, and is confirmed by the exact observations on other forms a dozen years after their mutative origin. Consequently the features in question may not be taken to be in any way the result of selection, but are in themselves new qualities.

Lamarck's evening-primrose offers such striking and easily recognizable examples

of discontinuous variation, and has been the object of so much detailed study, that we are in danger of giving way to the supposition that the mutation-theory rests upon the facts obtained from this plant alone. It is to be said, however, that if the evidence obtained from it and all of its derivatives were obliterated, the results of experimental studies which have been made upon mutations in other species, upon the behavior of retrograde and ever-sporting varieties, the occurrence of systematic atavism, and of taxonomic anomalies, pelories and other morphological features, would furnish ample support for the conception of unit-characters, and serve to establish the fact that mutations have occurred in a number of species representing diverse groups.

It is now becoming plainly apparent that the phenomena of hybridization, by the opportunities afforded for the study of the included unit-characters in a segregated condition; for the analysis of complex characters, and of the various principles governing the transmission, activity, dominance, latency and recessivity of characters promises to yield results of the first magnitude concerning the mechanism of descent and heredity. The possibilities, among plants, of crosses between species, comparatively widely different in morphological and physiological constitution, indicate that the ultimate generalizations upon hybridism will find a broader exemplification in plants than in animals.

It is pertinent to point out in this connection that the unguarded use of the terms 'variation' and 'mutation' to designate phenomena of segregation and alternative inheritance, when races or species are thrown together in a hybrid strain, is bound to result in much confusion, especially in dealing with plants, since it is well known that direct mutants of either parent occasionally occur in such mixed strains.

From this last consideration we pass naturally to a discussion of the nature of the material which may be of use in the study of fluctuating and discontinuous variability. It needs no argument to support the assertion that a successful experimental analysis of the behavior of separate characters may be carried out only when dealing with series of organisms fluctuating about a known mean with a measurable amplitude of variability.

Systematic species as ordinarily accepted generally consist of more than one independent and constant subspecies, or elementary species which may not be assumed to interbreed or intergrade, unless actually demonstrated to do so by pedigree cultures. So far but few elementary species have been found to interbreed. A due recognition of this simple fact would save us a vast amount of pyramidal logic resting on an inverted apex of supposition.

Again, the accumulation of observations upon the prevalence and effect of self- and cross-fertilization has totally unsettled the generalizations current within the last few decades. Briefly stated, a moderate proportion of the flora of any region is autogamous, a large proportion both autogamous and heterogamous, and a moderate proportion entirely heterogamous. The relative number of species included in the categories indicated varies greatly in different regions. To assert the deleterious effects of self-fertilization of all, or a majority of plants, is to base a statement upon evidence that lacks authentication, as has been strikingly demonstrated by recent results. As a matter of fact no phase of evolutionary science is as badly in need of investigation as that which concerns the effects of close- and cross-breeding.

It is also to be said that current misconceptions as to the extreme range of fluctuating variability of many native species

have arisen from a failure to recognize the composite nature of the Linnean, or group-species upon which observations have been based, as I have found with the common evening-primrose.

The demands of ordinary floristic work are usually met by the formulation of collective species, which are an undeniable convenience, and perhaps a necessity for the elementary teacher and the amateur. Upon the specialist in any subject rests the obligation to furnish his non-technically trained constituency with conceptions of the facts and principles within the domain of his investigations, which will be inclusive, and easy of comprehension. But if, in accordance with this requirement, the systematist contents himself with this looser, and with due regard it may be said, more superficial treatment, and does not delineate clearly the elementary constituents of a flora, or falters in carrying his analysis of relationships to its logical end, he fails notably in the more serious purpose of his investigations, and his work must be supplemented and extended before it becomes an actual basic contribution to the physiologic, or phylogenetic, branches of the science. To study the behavior of characters we must have them in their simplest combinations. To investigate the origin and activity of species we must have them singly and uncomplicated.

Lastly, we may turn to a phase of the subject which has, as yet, received nothing but speculative consideration—that of the causes which induce the organization of new characters and which stimulate their external appearance. The recurrence of the known mutants of Lamarck's evening-primrose and the occurrence of new mutants of other species have taken place in New York and Amsterdam under conditions that lead to the definite conclusion that a favorable environment, including the most advantageous conditions for

vegetative development and seed-production, facilitates the activation and appearance of latent qualities, and the inference lies near at hand that such conditions also participate as causes in the original organization of new unit-characters, or in changes in these entities. We conclude, therefore, that favorable environment promotes the formation of new species as suggested by Korshinsky, and that new species do not arise under the stress of infra-optimal intensities of external factors as proposed by Darwin.

Furthermore, it has been found that certain qualities arise and disappear more numerously, and presumably more readily, than others, in a mutating strain. Thus those embodied in the mutants *oblonga*, *lata* and *nanella* find external realization in many more individuals than those which constitute the differentiating features of *ruberinervis*, *scintillans*, *gigas*, *elliptica*, *subovata* and others.

Again, the inspection of the cultures made in Amsterdam and New York demonstrates that the last-named locality offers more favorable soil and climate for the evening-primroses. Correlated with this I am able to report that careful attention to the cultures has resulted in an increase of the proportion of mutants from the five per cent. maximum of de Vries to more than six per cent. in the last season, in the American cultures, and to say that some forms which did not reach maturity, and others which did not occur in Amsterdam, may find in New York a climate in which they may carry out their entire development. The cultures of Lamarck's evening-primrose now being carried on include fourteen recognizable mutants, and it is pertinent to state that I have mutants of other species which will be duly described after they have completed a cycle of development.

All components of the environment may

not be taken to be of equal value in the induction of new qualities, and I by no means wish to give the impression that the problem is on the point of being solved, but our hopes have been raised to the highest pitch that we may soon be able to discern the factors more or less directly concerned.

To be able to bring the causes that are operative in the formation and structural expression of qualities, *i. e.*, the moving forces of evolution, within the range of experimental investigation would be a triumph worthy the best effort of the naturalist; in that it would give us the power to give new positions to qualities and thus to produce new organisms, its importance would rank well with that of any biological achievement of the last half century.

SCIENTIFIC BOOKS.

The Zoological Record, Vol. XL., being records of zoological literature relating chiefly to the year 1903. Edited (for the Zoological Society of London) by DAVID SHARP. London. 1904. [Published early in 1905.]

The Zoological Record, which has now been published for forty years, is simply invaluable to the working zoologist. This statement seems so trite as to be ridiculous, but the fact is not known to some of those who most need the work. Some time ago I received letters from a well-known naturalist, asking for information concerning a group, the North American species of which he was cataloguing. In my replies I referred to the *Zoological Record*; but the answer came back: 'Do not refer me to the *Zoological Record*, it is not accessible to me.' I do not cite this as an unusual case; on the contrary, one continually observes that writers have not seen the *Record*, and have missed various things in consequence.

Consider for a moment what a time-saver the *Record* is, and how many oversights it prevents. Suppose I want to know about some genus of animals, literally *any* genus that may be mentioned; in half an hour I can find